acids; the passage from one class of alcohols, &c., to another; and very many other points of great scientific value, too commonly overlooked in the text-books, are here all carefully noted. As we said, the book requires attentive study, but this it will certainly repay.

M.M.P.M.

Thermal Paths to the Pole. An Address delivered before the St. Louis Mercantile Library Association in January 1872. By Silas Bent.

IN this pamphlet the author repeats the substance of a lecture delivered in 1868, the object of which was to show that the continuations of the warm Gulf Stream of the Atlantic, and of the Japan current in the Pacific, afford the only practicable avenues by which ships can enter an assumed open sea round the North Pole; and points out how the more recent Arctic explorations have confirmed the views then advanced.

The author's opinions should derive weight from the fact that he was one of the leading scientific observers in the American expedition of 1852, during which the Japan current was mapped out, and from his twenty-five years of observations at sea.

So far as the warm drift continuing the Gulf Stream into the Arctic region between Spitzbergen and Nova Zemlia is concerned, the theory of its influence (which, however, can hardly be called "original," since it has been current among Arctic authorities for many years) has indeed been remarkably confirmed in the past year by Payer and Weyprecht's voyage in open water to 79° N. lat. But it remains to be shown that the summer current from the Pacific through the narrow and shallow passage of Behring Straits has any considerable influence on the condition of the Arctic basin. In the circumpolar chart which accompanies the pamphlet, Behring Straits has been carefully widened to admit the Kuro-Siwo in a breadth quite equivalent to that of the Gulf Stream drift.

A considerable portion of the address is devoted to the description of a method by which "were it not for the inhumanity of exercising such a power," the whole of Europe might be placed at the mercy of America. Europe derives its mild climate from the Gulf Stream, and to divert this stream from its present direction would be to make "Europe a frozen wilderness." This grand result, the author believes, could be accomplished "by the possession of the Isthmus of Panama and the expenditure of half the cost of the recent war between France and Germany, in the excavation of a sufficient width and depth of the rock only that intervenes between the Caribbean Sea and the Pacific." Mr. Bent has himself, however, thrown some doubt on the entire practicability of his design by quoting, in a previous paragraph, the belief expressed by Professor Maury that the great mass of the Gulf Stream is formed by that part of the equatorial current of the Atlantic which passes to northward of the Antilles, and which "must be a hundred-fold greater than that which returns to the east from the Gulf of Mexico;" he has also omitted to notice that the force of the drift in the Caribbean Sea is not directed in any degree against the narrower portion of the isthmus, and we presume that even Mr. Bent would not attack the plateaus of Guatemala or Mexico.

K. J.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Spectroscopic Nomenclature

THE letter of Captain Herschel upon "Spectroscopic Nomenclature," which appeared in NATURE of April 25 contains many criticisms that are eminently just and timely, but there are one or two points in respect to which I should like to express dissent.

Thus as regards the name D_3 (to which he objects) for the yellow line of the chromosphere spectrum, it is not easy to see what other designation would better convey to the mind an idea of its position in the spectrum and its importance, without involving any assumption, or hypothesis even, as to the material causing it.

To say nothing of the fact that the whole Greek alphabet would not suffice to name one in three of the bright lines which have been observed in the chromosphere spectrum, there is this further difficulty, that if the letters are to be applied to lines in the order of discovery, ω is as likely to fall between α and β as anywhere else, but if according to position in the spectrum, then every discovery of new lines involves a revision of the nomenclature.

It seems doubtful therefore whether any better system is possible than to designate lines by reference to some standard map of the spectrum, as stars are catalogued by their right ascension and declination.

An accurate chart of the solar spectrum on which the lines should be mapped according to "inverse wave-length," proposed by Captain Herschel himself I believe, as well as by Mr. Stoney and others, would sufficiently resemble the spectrum seen in a spectroscope to be equally convenient in the observatory with that of Kirchhoff, and would be free from the reproach of arbitrariness and irregularity in its scale. Such a chart would be most gladly welcomed by all spectroscopists, and would immediately supersede those of Kirchhoff and Angström.

With reference to the green corona line, he writes "and now we have '1474.' No one knows what the true position of that line is. The line 1474 K is an iron line, and it is to the last degree improbable that the corona line is identical with it." I am not quite sure what is meant by the second clause. If only, that the position of this line may *possibly* (not by any means *probably*) be doubtful to the extent of ¼ of one of Kirchhoff's scale divisions, that is about ¼ the distance between the two E lines, I have nothing to object.

But if the sentence is intended, as one would naturally suppose, to convey the idea that the position of the line is not very accurately determined, and may be considered uncertain to the extent of several scale divisions, it is certainly wrong. I *know* of what I affirm, and perhaps may be allowed to refer to an article in this journal for Feb. 2, 1871, in which the evidence is stated as it was at that time, and it has received confirmation since.

Indeed as this bright line is almost always visible in the chromosphere to an instrument of sufficient power, I think I may confidently appeal to Mr. Lockyer or Dr. Huggins to bear me out in the statement that the bright scarlet line of the chromosphere appears to coincide no more perfectly with the dark C, than does this green corona line with the dark line at 1474 K.

I confess I am almost sorry that the spectrum of iron shows a bright line coincident with 1474, for all things considered, I cannot think that iron vapour has anything to do with this line in the spectrum of the corona, and the coincidence has probably only served to mislead.

But there are in the spectrum many cases of lines belonging to the spectra of different metals coinciding, if not absolutely, yet so closely that no existing spectroscope can separate them, and I am disposed to believe that this close coincidence is not accidental, but probably points to some physical relationship, some similarity of molecular constitution perhaps, between the metals concerned.

So in the case of the green coronal matter, is it not likely that, though not iron, it may turn out to bear some important relation to that metal? And yet I for one should be very glad if the application of higher dispersive power should show the apparent coincidence to be merely a very close juxtaposition.

C. A. YOUNG

Dartmouth College, U.S.A., May 16

Historical Note on the Method of Least Squares

THIS excellent method for the discussion of observations was published and first practically applied by Gauss in his Theoria Motûs, 1808. In the *American Journal of Science* for June 1871, Mr. Cleveland Abbe has shown that Prof. Robert Adrain, of New Brunswick, New Jersey, U.S., independently discovered the same method in 1808. I wish to call attention to what seems to me a singular oversight in the history of this subject, viz., to

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the fact that in 1770—1773, Lagrange published an elaborate memoir at Turin under the title "Mémoire sur l'utilité de la Méthode de prendre le Milieu entre les resultats de plusieurs Observations," &c. *Vide* "Œuvres de Lagrange," edited by J. A. Serret, vol. 2.

I have never seen any notice of this memoir except a translation of a part of it into German by Encke, published in the *Berliner Jahrbuch* for 1853. Thus in the abstract of a memoir by Mr. J. W. L. Glaisher, given in the notices of the Royal Astronomical Society for April 1872, the name of Lagrange does not occur.

I think that the English mathematician, Thomas Simpson, busied himself with this problem about 1750, but I am not able to refer to his works. ASAPH HALL

Washington, May 22

The Volcanoes of Central France

AN unlucky error, perhaps mine, in the letter on the "Volcanoes of Central France," p. 80, will quite prevent any reader finding the paper I mentioned of May 1865, which, instead of being in the *Gentleman's Magazine*, was in the *Englishman's Magazine*, a short-lived periodical, begun and ended, I think, with that year. As your two correspondents, Prof. Corfield and the Rev. Mr. Webb, like the writer of that paper, repeat the late Dr. Daubeny's most marvellous "conclusion" that there might have been nothing more eruptive in the phenomena than "bursting out of flames" from earthquake fissures, and even that the fires mentioned by Sidonius and Avitus might be "domestic conflagrations," may I briefly indicate the grounds that make these suppositions to me incredible? These fires, as named in the portions of each document that I have translated—quite distinct from the conflagration of some public building on the Easter festival of a previous year, which both writers afterwards relate at greater length as an earlier and less known case of successful prayer by Mamertus, the memory of which had encouraged him under these "prodigies" and "portents," the *ignes* (not *incendia*) that both writers make a chief or the chief part of the "terrors"—(Sidonius, indeed, names the earthquakes before them, but Avitus twice over puts the fires first)—these were *crebri* and *assidui*, continual for two or three years, yet not a word of what they fed on or what valuables they destroyed, and they were only *sæpe flammati*. Their being so sometimes is plainly named by Sidonius as an unusual and greater portent. Now, I never heard of any "domestic" fires that were *not* "*flammati*," whereas volcanic eruptions, even severe, seldom if ever involve flame truly so called, though their strongly illuminated smoke may often by night be mistaken for flames, and has led them to be called in extreme cases, as Sidonius here said, *sæpe flammati*. He adds that when thus "*flammati*" they did, or rather threatened to do, the only mischief named as even apprehended from them at the capital, the endangering frail roofs by a load of ashes thrown over, *superjecto favillarum monte*. Now, surely this is not an effect of any ædile conflagrations however often repeated (a repetition that would anywhere have been regarded rather as suspicious of incendiarism than as "prodigious" and preternatural). Nor would any such accidents lead Avitus to ask in his sermon to those who remembered all, "Who would not dread the Sodomitic showers?" Again, Mr. Webb conceives that earthquakes might not only drive the wealthier part of the population out of the city, "but, as it would seem, the beasts into it!" I never heard of shocks producing so singular an effect as driving any living thing into cities or buildings, and cannot conceive what natural event could so drive them, unless what is here by both witnesses implied, "Sodomitic showers" of hot or cumbering *favillæ*. Such showers, which we know to be often carried, from eruptions involving no lava, scores or even hundreds of miles, in the direction of the prevailing winds, would be carried from any of the well-known cones of the Forèz or Vivarais, towards, or even far beyond, Vienne; and wild animals, fleeing north-eastward, would have no refuge but under roofs; and if private house doors were habitually shut (as now in England) might crowd into the colonnades (*fori latera*) of that capital city. This incursion of the wild deer, bears, and wolves into towns was so well remembered as to become, in the later chroniclers, Gregory of Tours, &c., dwelt upon among the main "prodig es" of the time, along with the earthquakes and burnings of buildings, though any other fires cease to be implied; and the reason of this is obvious on comparing their accounts. They all copy one another, and the earliest, whose sole authorities were those two pompous and involved writers, mis-read them exactly as our moderns (except Sir F. Palgrave) appear to have done, confusing together the fires of the "prodigies," that led to the Rogation fasts with the earlier ædile conflagration at some Easter, said to have been prayed out by Mamertus, which occupies both the writer and preacher immediately after, and at greater length than these well-known "terrors" remembered by those they addressed personally.

The whole strikingly shows, as Sir F. Palgrave said, the fallacy of geological inferences from the "silence of history" (or what may be deemed silence) in times and places practically prehistoric, or at least preter-historic. He showed that, but for Pliny and a mere accident, we should probably have been as ignorant of even the Pompeii and Herculaneum eruption as of these equally attested ones. Again, the Spaniards would have preserved us no memory of the rise of Jorullo, in the very last century; and yet probably no part of Gaul in the generation when the Romans lost it was really more settled and populous than Mexico in its third century of Spanish rule. The only important colonies within moderate shower-range of the eastern volcanoes were Vienne and Lyons, the latter farther off, and not at that time a capital, indeed but little heard of in those early middle ages. And fires, not called damaging, only "prodigious" and terrifying to Vienne, and causing "Sodomitic showers" there, need not have been within a few miles, but far in the wilds, then hardly trodden, of its mountainous south-western horizon.

E. L. GARBETT

7, Mornington Road, N.W., June 1

Temperature of the Deep Sea

WILL you allow me to ask, through your pages, if there be any rule for ascertaining the temperature of the sea at given depths below the surface? To practical electricians such a rule would be very valuable.

I will state a case. There is a submarine cable connecting two stations, A and B, 150 miles distant. The temperature at A is 75° Fah.; that at B, 68° Fah.; and the average depth at which the cable lies 120 fathoms: what is the average temperature of the cable?

If you could refer me to any work in which this point is treated I shall be obliged. F.

ENDOWMENT OF PROFESSORSHIPS

THE following correspondence between Professors H. E. Roscoe and B. Stewart, of Owens College, Manchester, and the Chancellor of the Exchequer, is published in the *Times* of Monday last:—

"TO THE RIGHT HON. ROBERT LOWE, CHANCELLOR OF THE EXCHEQUER.

"Owens College, Manchester, May 21, 1862.

"SIR,—In the *Times* of May 17 you are reported, at the presentation for Degrees at the University of London, to have pointed out 'how the endowment of Professorships naturally tended to make teaching inefficient (seeing that the revenues come in independently of the results of teaching), suggesting that those who had any money to spare for the advancement of education should rather make it available in the forms of Scholarships and Exhibitions.'

"While we gratefully acknowledge the many services which, as Chancellor of the Exchequer, you have rendered to the cause of knowledge, we yet feel most strongly that the above expressions are calculated to mislead, and that were your suggestions to be carried out, the result would be fatal to the higher education of this country.

"We therefore request permission to lay before you our own views on this most important subject. Writing from the very house once inhabited by Cobden, we feel proud to be connected with a city which was the birth-place of Free Trade; yet we feel equally privileged to form part of a very useful institution which never could have existed

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